Art. I.—*The Theory of Temporary Stars.*

By E. F. J. LOVE, M.A., F.R.A.S.

[Read 13th March, 1902.]

1.—Introductory.

The two rival theories concerning the origin of temporary stars—or *Novae*—respectively ascribe their phenomena to collision and to eruption. Till within the last three years the collision theory was preferred by astrophysicists, a large majority of them deeming the spectroscopic evidence conclusive in its favour; but the publication by Wilsing[1] of his famous memoirs "On the interpretation of the typical spectrum of new stars," and "On the effect of pressure upon the wavelength of the lines in the hydrogen spectrum," put a new face on the matter by showing that the emission from the star of gases under pressure was competent to account for the spectroscopic phenomena. Of special importance was his experimental proof of the fact that the dark lines must, on this hypothesis, always be more refrangible than the corresponding bright ones, since this fact was one of the principal difficulties in the way of the collision theory, or for that matter of any reasonable mechanical explanation of the typical spectrum, a difficulty emphasized of recent years by Mrs. Fleming's discovery of a considerable number of *novae* on the Draper memorial photographs.[2]

Such was the state of the question when *Nova Persei* made its appearance on 21st February, 1901. Owing partly to its great brilliancy—even in the latest stages discussed here it was still very little below the 6th magnitude—partly to the improvement in photographic processes, the transformations of this star have been studied with a degree of closeness, accuracy and continuity never approached in the case of other *novae*. Naturally each investigator attempts to explain his results in terms of the

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[1] Ap. J., x., pp. 113, 269.

[2] See Pickering (Ap. J., xiii., p. 173) for a list of these.

theory which he favours most; my object, therefore, in writing this paper is to discuss the work done on the star at different observatories during last year, in order to see what light it throws, taken as a whole, on the claims of the rival theories.

2.—Early History of the Outbreak.

Nova Persei was the first of the temporaries to have its spectrum examined before and during, as well as after, its epoch of maximum luminosity. Discovered by Anderson on 21st February, its spectrum was photographed by Pickering[1] at Harvard on the 22nd; by Vogel[2] at Potsdam, as well as at Harvard on the 23rd—the night of the maximum, when the star for a short time held the proud pre-eminence of "the brightest star in the northern heavens"—and at the Lick[3] and Yerkes[4] observatories, as well as at Harvard on the 24th; all other photographs known to me are of later date.

These earliest photographs prove that on 22nd February the spectrum was that characteristic of the "Orion group" of stars; it showed a continuous spectrum crossed by a comparatively small number of fine dark lines, practically undisplaced from their normal positions, among which those of hydrogen and the magnesium line λ4481 were decidedly the most prominent; Pickering records these as "bright on the less refrangible edge;" there was no sign of the typical spectrum. One the next night Pickering found K present; otherwise the spectrum was "apparently unaltered," but the star was obscured by clouds, and the photograph faint. On the same night, however—and, judging from the difference in longitude of the two observatories, some time previous to Pickering's photograph—Vogel obtained two plates, showing a bright continuous spectrum, crossed by broad hazy hydrogen absorptions,[5] and by a number of fine dark lines, H and K being conspicuous among the latter, but he

1 Ap. J., xiii., pp. 170, 232.

2 Ap. J., xiii., p. 217.

3 Ap. J., xiv., p. 269.

4 Ap. J., xiii., p. 238.

5 Possibly the cloudy weather prevented these from showing on Pickering's second plate.

found no trace of bright lines or bands on his plates; the broad hazy absorptions were displaced towards the violet by an amount corresponding to a velocity of 700 kilometers per second towards the sun. By the 24th the typical *nova* spectrum was fully established. It differed slightly from that of *Nova Aurigae*, showing the bright bands much broader, and the dark ones less dark relatively to their surroundings, than they appeared in the spectrum of the earlier star.[1] These bands, in addition to the lines of hydrogen, included the "enhanced lines" of iron, calcium and magnesium, and (possibly) D_3; H and K showed both as bands and as fine lines.

3.—Bearing of the Earliest Observations on Theory.

This may be summed up as follows:—

1. The star, when near the maximum, was certainly exceedingly hot. The coincidence of the first spectrum with that of the Orion stars establishes this fact, which is even better attested by its great extension into the ultraviolet and the high relative intensity of that portion. The presence of "enhanced line" absorptions proves the same thing.

2. The change to a typical *nova* spectrum, though rapid, was gradual. Pickering, on the basis of his own photographs, regards it as "very sudden"; but the coincident work of Vogel shows that the dark bands made their appearance before the bright ones, and that at first they were feeble; consequently the setting up of the typical spectrum seems to have had nothing catastrophic about it.

3. Pickering[2] calls attention to a possible *experimentum crucis* for deciding between the rival theories. He points out that, on the collision hypothesis, the maximum separation of the bright and dark bands should occur at the epoch of maximum brightness, and considers that the comparison of his second and third photographs shows that the separation had increased between 23rd and 24th February—*i.e.*, after the maximum was passed—and consequently the collision hypothesis cannot be true.

[1] Pickering (Ap. J., xiii., Plate iv.) reproduces the spectra of the two stars.
[2] Ap. J., xiii., p. 277.

Of course his argument falls to the ground if, on the night of 23rd February, the bright bands were absent from the spectrum. It seems to me that the presence of the bright edges of the fine absorptions, recorded by Pickering, may be regarded as doubtful, seeing that Vogel got no trace of them on plates taken under better atmospheric conditions; the latter observer, indeed, is of opinion that a large part of the bright light of the *continuous* spectrum of that night was due to very dense hydrogen. If this be the case, these observations, taken alone, do not furnish the *experimentum crucis*, which will have to be discussed —if at all—on the basis of evidence furnished by the later observations.

4.—Second Stage of the Star's History.

While the typical spectrum was predominant, a number of photographs of the spectrum were taken at the Lick[1] and by Lockyer at South Kensington,[2] which demonstrate the important fact that the wavelengths of the bright hydrogen lines increased progressively in value for a time, while the wavelengths of the dark lines—which were much shortened—remained nearly stationary in magnitude. As Lockyer gives no measures, but merely records the fact that a change did occur, I have put together the scattered measures of the Lick observers in Tables I. and II.

TABLE I.—DARK BANDS.

Line	Normal λ.	Feb. 25.	Mar. 13.	Mar. 18.	Ap. 1.	Ap. 5.
Hζ -	3889	—	3870	3870	—	—
Hε -	3970	3952	3950	3950	—	—
Hδ -	4102	4084	4081	4081	4100	4103
Hγ -	4341	4322	4318	4319	4340	—
Hβ -	4862	4838	4835	4837	—	4861

1 Ap. J., xiv., *loc. cit.*

2 M. N., lxi., App., pp. 15, 21, 37.

TABLE II.—BRIGHT BANDS.

Line.	Normal λ.	Feb. 25.	Mar. 13.	Mar. 18.	Ap. 1.	Ap. 5.
Hζ -	3889	—	3896	3893	—	—
Hε -	3970	—	3974	3975	3971	3972
Hδ -	4102	4094	4105	4108	4102	4102
Hγ -	4341	4335	4342	4347	4341	4342
Hβ -	4862	4853	4863	4869	4864	4861

From these figures we see that the dark bands were at first strongly shifted towards the violet, regaining their normal positions about the beginning of April. The bright lines, too, were at first displaced towards the violet (though less than the dark ones) but by the middle of March this was exchanged for a much smaller displacement towards the red, the lines returning to their normal positions at about the same time as the dark ones. In the April photographs, therefore, the characteristic paired lines are no longer seen, being replaced by broad bright bands in the normal positions, upon which broad hazy absorptions are mesially superposed.

The gaps in the Lick series of photographs are bridged by those taken at South Kensington, as well as at the Yerkes[1] and Stonyhurst[2] observatories. These agree with the Lick photographs in showing that early in March the bright lines were displaced towards the red; while the South Kensington plates show that on 26th February[3] the displacement either way—if it existed—must have been small. On the other hand, the Stonyhurst observations tend to show—in opposition to the Lick observations—that the bright-line displacements remained unchanged from 3rd March to 26th April; it is, however, to be noted that Father Sidgreaves' method of comparison is much better suited to differential measurements than to absolute

1 Ap. J., xiii., pp. 173, 238.

2 M. N., lxii., p. 137.

3 It must not be forgotten that South Kensington is more than ten hours ahead of the Lick Observatory in longitude; a fact of fundamental importance where possibly rapid changes are concerned.

determinations of wavelengths,[1] while Lockyer's direct comparison, on 25th April, between the Hβ and D_3 lines of the *Nova* and those furnished by vacuum tubes, showed the wavelengths on that date to be normal. The Yerkes plates also, to a certain extent, support the Lick results; seven plates taken before 22nd March gave for the wavelength of Hϵ 3973, six plates subsequent to that date made it 3970. The cumulative evidence is strongly in favor of the accuracy of the Lick determinations of the absolute wavelengths of the bright lines as against those of Stonyhurst.

There still remains a difficulty about the dark lines. The Yerkes and Stonyhurst observers agree in representing them as dying out *in sitû*, and completely disappearing before the end of March, owing possibly to decay in brightness of the continuous spectrum. It is, however, noteworthy (1st) that these bands had each of them several components, the more refrangible of which disappeared first, and (2nd) that Lockyer's simultaneous observation and plotting of the intensity curves for the bright bands shows their maxima as shifting from the violet towards the red side of the bands. In other words, the phenomena, taken as a whole, seem to indicate a gradual coalescing of the bright and dark bands, such as the Lick observers found completed in the beginning of April.

We must not lose sight of the *fine* dark lines due to metallic absorptions, which were examined with special care by the Lick observers. H, K, D_1 and D_2 were present as very narrow, sharp lines, side by side with the broader absorptions of calcium (and sodium ?). These narrow lines were very slightly displaced towards the red, and agreed—within the limits of experimental error—in showing that their source was recedimg from the sun with a speed of approximately 7 kilometres per second, this value being steadily maintained, up to the middle of August,[2] quite independent of the changes in either the heavy absorptions or the bright lines.

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1 A fact of which he himself appears to be fully conscious. It may be added that he considers a good many of his plates to have been over-exposed to such an extent as to unduly broaden the lines. Can this have produced slight displacements of the maxima, which we know were never central?

2 When the last recorded measurements were made.

5.—Bearing of the Second Stage Phenomena on Theory.

1. If we desire to express these phenomena in terms of the collision theory as usually presented, we must proceed as follows :—

The sources of the bright and dark bands possessed, before and during maximum, little or no relative velocity in the line of sight ; two days later they were moving in the same direction—towards the sun—but with different speeds ; a fortnight later they were moving in opposite directions with the enormous relative velocity of 1800 kilometres per second ; and after another fortnight both came to rest. Meanwhile, another body concerned in the phenomena was travelling slowly away from the sun with uniform speed.

I cannot imagine how such a series of performances is possible ; neither collision nor orbital motion fits the facts. But it does seem to me that we have here as strongly marked a case of Pickering's *experimentum crucis* as Pickering himself could desire ; consequently, as far as *Nova Persei* is concerned, we must abandon all such explanations of the spectrum as are based on the assumption of relative motion of different bodies.[1]

2. Wilsing's hypothesis fits the facts very well. The variations, both in magnitude and direction, of the linear displacements are readily explicable as due to pressure changes ; we need only suppose that in the earlier stages the pressure of the incandescent gases was comparatively low, then rapidly increased, remained almost stationary for a time, and finally fell off ; while the fact, attested by all the observers, of the multiplicity of the maxima in both bright and dark bands is readily explained by the very probable supposition of different pressures in different parts of the gaseous layer—pressures, moreover, which can in all cases be estimated in numbers of atmospheres by no means unreasonably great.

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[1] Had the distance between the bright and dark bands remained constant, the deductions would have been different.

6.—Later History of the Star.

On March 19th the *nova* entered upon the third stage of its life history, so far as we can ever know it. The decline in its luminosity, previously very rapid, became extremely slow; but the star now assumed the character of an irregularly periodic variable of rapidly lengthening period, its brightness fluctuating between the 4th and 6th magnitudes.[1] With this variation in brightness was associated a variation in the spectrum, which showed two well-marked types; the first type agreed with the spectrum already described, save that the metallic lines were steadily weakening; the second type differed from the first by the absence, or at least the great relative weakness, of the continuous spectrum, the absence of dark bands, and the gradual emergence of new bright ones. The conditions of change from one spectrum-type to the other were very carefully studied by Sidgreaves[2], who found that it was associated with the absolute brightness of the star, and not with the phase of the light-variation; whenever the magnitude sank below 4·57 the second type appeared, whenever it rose above that value the first type was re-established. Pickering[3] to a certain extent supports this conclusion, as all the second type photographs secured by him, with one exception, were taken at minima; the exception is important, as he records that "the magnitude of the star was the same on 12th and 13th April, but the spectrum was different;" there appears, however, to be a possibility of error on his part, as the light-curves of Lockyer and Child agree in showing that, on both dates, the brightness was near to Sidgreave's critical value, being slightly below it on the 12th and above it on the 13th; the same thing is shown by Rambant's observations[4], which give the magnitude on 12th April as 4·67, and on 13th April as 4·49; on the whole, Sidgreaves seems to be right.

The most noteworthy of the early changes in the spectrum of the second type are the disappearance of the "enhanced" metallic lines and the gradual emergence of characteristic nebular

1 Lockyer (M.N., lxi., App. p. 59) and Child (M.N. lxi., p. 483) give drawings of the light curve for February, March, and April.

2 *Loc. cit.*

3 Ap. J., xiv., p. 80.

4 M.N., lxi., p. 467.

lines together with those of helium[1]. Among the nebular lines, λ 4363 made its appearance on 22nd March[2], while λ5007 was certainly, and λ3869 possibly, observed on 28th March, the two latter showing themselves as extensions towards the violet of λ5019 and $H\zeta$; on this last point all the observers are in agreement.

By 19th June, owing to the gradual dying out of old lines and development of new ones, the bright-line spectrum had closely approximated to the nebular type[3]; the periodic variations had meanwhile largely died out, though the steady fall in brightness[4] still went on.

The apparent transformation of the star into a nebula was not a surprise, as the same fate had befallen *Nova Aurigae*; owing, however, to the faintness of that object, the process in its case could not be watched, and was only ascertained to have taken place several months afterwards, when the *nova*, for some unknown reason, experienced a great revival in its light. *Nova Cygni* is also known to have undergone a similar change, but in its case—though the measurements were not precise enough to allow any dogmatising on the subject—it seems likely that the two principal nebular lines were present in the spectrum from the first, which in the early spectrum of the other two they certainly were not.[5] It may be added that, as far as the scanty observations go, *Nova Aquilae*[6] appears to have had a similar history to *Novae Aurigae* and *Persei*, a history which we may, therefore, regard as typical of *novae* in general.

By the beginning of August the bright-line spectrum of *Nova Persei* had fully assumed the nebular type, so far as the wavelengths of the lines were concerned; moreover, five nebular lines of unknown origin (among which three, forming a group, of wavelengths 3869, 3968[7] and 4363, are characteristic nebular

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1 Lockyer, M.N., lxi., App., p. 37.

2 Sidgreaves, *loc. cit.*

3 Pickering, Ap. J., xiv., p. 81.

4 Carefully recorded by Gore, M.N., lxii., p. 156.

5 The spectrum of *Nova Cygni* afterwards reverted to the stellar type (Harvard Observatory Annual Report, 1879-80, p. 7) just as those of *Novae Cassiopeiae* and *Coronae* appear to have done. Lockyer asserts (M.N., lxi., App., p. 19) that *Nova Cygni* is now a nebula. What his authority for the statement is I do not know.

6 Pickering, Ap. J., xiii., p. 172.

7 The Lick observers show conclusively that these must not be identified, as they have been in the past, with helium and hydrogen lines.

lines, while λ5007 and λ4959 are the "chief" nebular lines) together with the helium line λ4713, were the strongest lines in the spectrum. Omitting λ4959, these appear *alone* in the reproduction[1] of one of Sidgreaves' plates, showing that they were then brighter than the hydrogen lines lying near them.

But a comparison of Sidgreaves' results with those of the Lick observers shows that in some respects the spectrum differed widely from that of a typical nebula. In the first place, the relative intensity of the lines was always altering, and was never that of a typical nebular spectrum. Throughout August and September Sidgreaves[2] and Lockyer[3] found that λ3869 was the brightest line in the spectrum, though λ5007 is not merely the brightest line in the spectrum of ordinary nebulae, but absolutely dwarfs all others. However, by 6th October, when Sidgreaves brought his series to a close, $H\beta$ and $H\gamma$ were the only surviving hydrogen lines, while λ5007 and λ4959 had asserted their superiority both over them and over λ3869, though not to anything like the normal extent.

Another pronounced difference between the two spectra was the great width and diffuseness of the lines, combined with their complex structure. This showed that the hot gases, though similar in chemical nature to those of nebulae, were in a condition of far greater density. This is further borne out by the fact that, of the helium lines present, every one without exception[4] belongs to Kayser and Runges' "high density" series.

A further difference of a very important character consisted in the continued existence of a continuous spectrum by no means faint for a 6th magnitude star,[5] crossed by the old series of narrow dark lines, the displacements of these lines indicating the same steady but low velocity as they had done early in the year.

The discovery by Ritchey,[6] on 20th September, of a fairly large nebula surrounding the star, falls within the period covered by the later spectrographic researches. In itself a most interesting

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1 All the lines are stated by Sidgreaves to be visible on the negative, but obviously those wiped out in the reproduction must be the weakest.

2 *Loc. cit.*

3 P.R.S., lxix., p. 137.

4 Shown by a table given by the Lick observers; *loc. cit.*, p. 290.

5 Sidgreaves, *loc. cit.*, p. 150.

6 Ap. J., xiv., p. 167.

—and quite unexpected—phenomenon, its interest pales in comparison with that of the changes subsequently found to be apparently going on in it; for a photograph taken seven weeks later[1] shows that the nebula was, to all appearance, rapidly expanding in all directions, and that one portion of it, apparently in contact with the star, had greatly increased in luminosity during the the interval, while the rest of the nebula had lost much of its light.[2]

7.—The Theory in the light of the later observations.

In this section there are many points for discussion. I have, as above, distinguished them by numbers.

1. Variations in brightness *per se* are readily explicable on the collision theory by attributing them to successive encounters, as Lockyer and others have pointed out; they are equally easy to explain on the eruption theory, by ascribing them to alternate expansions and contractions of the gaseous matter, consequent upon the checking of the outrush. Of the two hypotheses, the second accounts satisfactorily for the quasi-periodicity of the light-variations, which are pretty much what we should expect if it were true[3]; while unless we are to assume a quasi-periodicity in space of the structure of the invaded system, similar to the quasi-periodicity of the light-curve, it is difficult to see how the theory of successive collisions can be made to fit the facts; a theory which necessitates any such subsidiary hypothesis is, to my mind, self-condemned.

2. Neither theory can be regarded as satisfactorily accounting for the change of spectrum from a stellar to a nebular type.[4]

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1 Ap. J., xiv., p. 293.

2 The question obviously suggests itself—Is the nebular spectrum due to the star or to Ritchey's nebula? The answer, I take it, must be that it is due to the star, otherwise we should find on the plates a narrow stellar spectrum crossed by long bright lines, just as in Huggin's photograph of the combined spectrum of the Orion nebula and the trapezium stars; but nothing of the kind appears to have been observed. Besides, the nebula is too faint an object to give *any* spectrum in the time allotted to the exposures by the spectrographers.

3 The light curve closely resembles the displacement curve of a damped vibrating system.

4 Lockyer's meteoritic hypothesis might have done so, had the spectrum passed through some or all of the stages characterised by him as Polarian, Aldebarian, and Antarian (M.N., lxi., App., p. 19) before assuming the nebular type; but this, apparently, it failed to do.

Sidgreaves' work shows that the change from the first to the second type—which probably marked the inception of the nebular type—was pretty sharply associated with a definite level of temperature; probably, therefore, the assumption of the complete nebular type corresponds to a lower temperature level. Why a nebular spectrum should be associated with relatively low temperature is quite another question; possibly the two rival theories are not concerned in the answer to it, which may be a matter of chemistry alone. Too much stress must not be laid on the coincidence in point of time between the periodic variation of light and the appearance of the nebular lines, seeing that in the case of *Nova Aurigae* the variable period came to an end long before any nebular lines showed themselves; moreover, in the case of η*Argûs* the variable period *preceded* the maximum epoch, while in that of *Nova Cygni* there is no evidence for its existence.

3. The discovery of a nebula surrounding the star lends plausibility, at first sight, to the hypothesis of successive collisions, and was naturally claimed in support of it; but it seems to me that the details of Ritchey's investigation tell in a different direction. The rapid brightening (alluded to in §6) of the patch of nebula in contact with the star might of course have resulted from a new collision, the evidence of which reached us between 20th September and 11th November; but in that case we might reasonably expect to find that a rise in luminosity of the star occurred somewhere between these epochs. Nothing of the kind was, as a matter of fact, observed.

4. We must now consider the unique problem offered by the observed expansion of the nebula.

Taking Ritchey's measurements of the distances between the star and the principal nebular condensations, we are led by simple inspection to conclude that the rate of expansion was the same all over the nebula, these distances increasing by about one-seventh of their total amount in fifty-four days. This is well shown in Table III., in the first column of which we have the letters used by Ritchey to designate the principal condensations, in the second and fourth the angular distances of these from the star on two dates, expressed in seconds of arc, in the third the quotients obtained on dividing the numbers in the second column by 7, and in the fifth those obtained on dividing the numbers in the fourth column by 8.[1]

TABLE III.

Condensation.	Sep. 20.	Nos. of previous column ÷ by 7.	Nov. 13.	Nos. of previous columns ÷ by 8.
a	430″	61½	497″	62
b	374″	53½	420″	52½
c	346″	49½	381″	47½
d	350″	50	395″	49½
e	371″	53	427″	53½
f	366″	52	378″	47

The agreement between the third and fifth columns of Table III. is very striking,[2] and fully warrants us in assuming that the rate of expansion at all parts is pretty much the same. The difficulty lies in the interpretation of this fact, not in its establishment.

Kapteyn[3] and Wilson[4] have independently arrived at the conclusion that what we see is not a motion of the nebulous matter, but the light of the outburst reflected successively from more and more distant parts of a pre-existing nebula; Wilson has computed the distance of the *Nova* on this assumption, and finds it to be about 250 light-years. The hypothesis accounts very well for the agreement between the rates of expansion at different points, and obviates the ascription of enormous velocities to masses of matter; nevertheless it appears to me that there are serious difficulties in the way of its acceptance.

In the first place, the apparent rate of expansion was too slow to be consistent with it; the time interval from 23rd Feb. to 13th Nov. is 263 days, that from 23rd Feb. to 20th Sept. is 209

[1] Ritchey's method of measuring these distances on enlarged photographs, by means of a scale, is obviously better suited to the determination of ratios than of differences; hence the method of comparison adopted above, which uses the actual measurements, and neglects the differences tabulated by Ritchey.

[2] The only discrepancy of any real importance is that furnished by condensation *f*.

[3] Astr. Nach., No. 3756.

[4] Nature, 2nd Jan., 1902.

days; had we, however, used 263 and 209 (or 5 and 4) as divisors in drawing up Table III., instead of 8 and 7, the discrepancies between the third and fifth columns would have been serious.[1] Another difficulty is the brightening up of the central patch of nebula, already mentioned as an obstacle in the way of the collision theory; to explain it on Kapteyn and Wilson's hypothesis we should certainly have to assume an increase of the star's luminosity at a late stage of its history, which, as we have seen, probably did not occur.

If we are prepared to go the length of supposing that the substances forming the nebula were ejected from the star, we shall find that this hypothesis accounts, better than the others, for the facts above-mentioned. We may regard it as practically certain that the rate of radial expansion of emitted gases would diminish as the volume increased, hence the objections to Kapteyn and Wilson's theory, based on Table III., do not apply to this one, but rather tend to support it. Again, the brightening of the central patch of nebula furnishes no difficulty to the ejection hypothesis; it may well be that the outflow from the star, though immensely slowed down, is still going on—possibly is intermittent; but intermittence in the outflow does not demand the same variation in luminosity as a new collision or an actual revival of light, indeed the intermittent outflow would probably hold the light-variations in check. In suggesting this alternative hypothesis, I am fully conscious of its extravagance; this, however, is scarcely as great as it looks, on the contrary the enormously rapid outgrowth of the tails of bright comets—which are probably formed under the action of forces immeasurably weaker than those at work in a *Nova*—seems to give it some degree of plausibility.

The question can hardly be settled until the star has been examined for parallax. If this prove to be measurable, Kapteyn and Wilson's hypothesis would be at once disproved; while the ejection hypothesis would be somewhat strengthened, since the

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[1] Kapteyn and Wilson both suggest that the discrepancy in the values of the star's distance furnished by the earlier and later photographs *may* be due to the inclination of the nebula to the line of sight; but this can, I think, hardly hold good in the case of a body whose greatest angular diameter is only a few minutes of arc. However that may be, the objection stated above is a grave one.

velocities it demands would then be of the same order of magnitude as those concerned in the formation of comets' tails. If, however, the parallax be too small for measurement—as has been the case with all *Novae* yet investigated—the ejection hypothesis would be discredited, while the other grave objections to that of Kapteyn and Wilson would still hold good.

8.—Conclusion. The Origin of Temporary Stars.

The evidence collected in this paper does, as I think, disprove the theory which attributes the spectroscopic phenomena to the relative motion of different bodies, and shows them to be well explained by ascribing them to disturbances in a gaseous mass. That being so, we must go further, and admit that the spectroscope throws no light on the origin of *novae*; since it gives us no inkling of the cause which impelled the star to suddenly rise enormously in temperature and then clothe itself with fiery vapours.

The origin of the *novae* must therefore be discussed from considerations other than spectroscopic. My own view is that nothing we know of but collision is competent to originate a *nova*; the only other assignable cause is chemical action within the star, and I do not see how to reconcile a chemical hypothesis with the facts, seeing that the star attained a temperature such as probably no compound could experience without decomposition. This conviction has been materially strengthened by a conversation with Professor Lyle, who gives it as his opinion that—even in the most favourable case—the generation of heat by chemical action alone could not have been rapid enough to account for the enormously sudden rise in luminosity.[1]

The final conclusion is, therefore, the following:—

A collision—the direct evidence of which is not forthcoming—took place in the heavens, resulting in a great development of heat in the colliding masses; this was followed by a tremendous outpouring, in all directions, of incandescent gases. The subse-

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[1] Williams's photographs of the region in the heavens in which the outbreak occurred show (M.N., lxi., p. 337) that the star was below the 12th magnitude on 20th February; hence its light must have increased *at least* 5000 times in less than 28 hours.

quent history of the ejected gases is disclosed to us by the variations of the bright and dark line spectra, that of the main body by the dying down of the continuous spectrum. These combine to show that the star has throughout retained the general character of an Orion—or at least of a Sirian—star, while the mass of gas surrounding it now differs from an ordinary nebula mainly in its state of aggregation. The exact relation between the outbreak and the changes going on in Ritchey's nebula is a problem for the future to solve.